Response to the reviewer #3 comments.

Thank you very much for the detailed comments! We answer them below one-by-one.

However I think the quality of the manuscript could be improved if the authors could run a more comprehensive comparative study with current state of the art parameterization. According to the conclusion, the manuscript is aiming to compare the new approach with existing stack-oriented models and Plume Rise Model (PRM). However they did not consider the use of the PRM developed by Freitas et al 2007, 2010, or the mass flux formulation of Rio et al 2010. The only 1D PRM used in this study is the BUOYANT model which gets less attention than the Briggs equation in the section 2 on 'formulation' description. For example, I did not find any details on the parameterization of fire heat emission in BUOYANT. Furthermore the way the work on PRM comparison is carried along the paper is rather confusing: (i) the abstract did not mention it and (ii) rather quick assumption are made on PRM behaviour. For example at the end of section 5.3, the discussion on the latent heat sensitivity of the Freitas model is unfunded. The effect of latent heat (responsible of a second updraft in the case of some fire as the one studied in Freitas et al 2007) cannot be used as the main reason for the over-estimation of the injection height. Other parameters like the fire heat emission evaluation or the entrainment scheme might be as much relevant in this issue.

In conclusion, I have no major concerns and recommend publishing the manuscript in ACP, however I think that in order to improve the quality of the paper, its structure should be rethought to make its claims and its results more consistent. The comparisons with 1D PRM formulation should be dropped or if the authors want to keep it further work need to be done.

The selection of BUOYANT is based on the similarity of the formulations of this 1-D model with that developed by Freitas et al. As stated in the paper, the only difference between these models is the latent heat transport and release during the phase transition of water vapour. We admit this difference and have pointed out the tendency towards under-estimation of BUOYANT in comparison with the over-estimation of the Freitas' model (admittedly, based on indirect information reported by Pfister et al). This work has to be continued and we are going to initiate an extensive intercomparison exercise involving major research groups behind the modern plume-rise tools.

For the needs of the current paper, we improved the comparison with PRM in the following way:

- abstract is corrected and BUOYANT model is introduced there
- discussion n section 5.3 has been modified and effects of entrainment and fire heat release are extended

***SPECIFIC COMMENTS**:*

***Abstract**:*

As stated previously, the abstract never refers to the work done on 1D PRM.

The reference has been added

***Section 2** - Existing Plume rise Formulation:*

p27942 -l15: VSMOKE is not used in the reference paper Freitas et al 2007.

Missing comma and a ref. Corrected

Section 4 – Methodology for injection ...

p27944-l3-6: The vertical updraft near the flame and several hundred meters above the fire is much more important than the atmospheric fluctuation. Riggan at al 2004 measure vertical wind speed of 15 m/s (8 m/s) over cerrado fire at an altitude of ~200 m (~1,000 m). At these altitudes the momentum is definitely not negligible and the entrainment of fresh air from the ambient environment plays an important role. A direct consequence of this mechanism is the 'puff' structures formed at the edge of the plume. I understand that the formulation described here is making some assumptions which are acceptable; however the author should consider reformulating the sentence of line 5.

The confusing sentence has been removed: we do not need to neglect the whole momentum - but only the effect of non-zero momentum to friction of the plume. The energy excess $e(z)$ can be considered as a sum of thermal and kinetic energies. We explicitly ignore friction in the eq.(4) – but it is stated there.

Equation4: I did not understand the derivation of this equation. I could be wrong, but according to the text p27945-line 2 the first term describe the work against the buoyancy force. Therefore I was expecting a term similar to the one used in the common formulation of the CAPE, -g B dz. According to the structure of Equation 4, it seems that the first term is coming out of 1/v (d E/dz) using which is the only contribution of sensible heat which is in contradiction with the definition of P_f *in the formulation of Equation 3 (see also comment posted by Edward Hyer). However this doesn't explain the negative sign. Could the author give more details on the derivation of this equation? Furthermore, the presence of* E_0 *in the second term instead of E is also surprising. Does this means that the only parameter varying with altitude is the cross section S of the plume?*

Well, the situation is somewhat different from that of CAPE: we deal with the overheated plume with unknown initial temperature rising through generally stable atmosphere. Therefore, we used only the idea but followed different derivation path, which is now explained in details.

The second term is indeed with E_0: in absence of friction the energy is conserved. This is the very simple model, from where we need only semi-qualitative analytical dependence. The rest is handled by the calibration procedure.

Equation 6: Could the author check that there is no missing factor 1/w in the second term of the right hand side. Adding this term I ended up with a different formulation for equation 7, which yields to

However I think this does not alter the discussion of page 27946.

Misprint, corrected. Further equations are OK.

Section 5 – Inter comparison

Equation 14: This equation refers to the Dozier (1981) algorithm and no reference is made to this previous work. Furthermore I think the equation reported here is wrong. The radiance L_i *emitted by a pixel at frequency* v_i *is* $L_i = B(v_i,T_{rad}) = p_f B(v_i,T_f) + (1-p_f)B(v_i,T_b)$*, where* p_f *is the proportion of the pixel with fire and B the plank function .*

The equation is indeed, from Dozier, the missing reference is added. It is correct, however: we only explicitly named burning and background areas instead of the p_f fraction.

The author should also give some more details on the nature of the MOSID data used here. Is it the MOD14 product? Did they apply any atmospheric correction to the brightness temperature T_{rad}*? How the background temperature was evaluated?*

Clarification added.

P27949,l15: If Tf ~ Tb, it is more likely that the pixel won't be detected as a fire pixel than the Dozier algorithm would not converge. The authors can refer to the work of Giglio and Kendall (2001) for more details on this issue.
Reformulated. However, we did see a few similar fire and background temperatures in the dataset.

Equation 15: Would that be possible to give more details on the formulation of the buoyancy flux F. Variables r and v_s are not defined.
In fact, they have been introduced in the eq (1), where the initial formulation for F is also given. The reference is added.

p27951,line 10: The reference to 'it' is not obvious.
Corrected

p27951,line 16-17: Freitas et al 2010 show that ambient wind shear can have an impact plume height.
p27951,line 16-17: As stated in the 'general comment' section, such sentence does not present solid ground as it mainly relies on one single plume studied in Freitas et al 2007 that might not be representative of the 2000 plumes overlooked in this work.
We added the reference and reformulated the discussion to more accurately represent the state-of-the-art. The direct link of the overstated fraction of the FT plumes to latent heat inclusion was removed. However, despite our best efforts, we have not found any correlation of the plume height and wind speed picked at any altitude. There can be many reasons for that and we tried to discuss these in the paper. However, there is evidently a need for further research in this area.

Section6 - Discussion
To highlight the improvement of the new parameters (Eq. 21) in FT fire detection, it would be interesting to see a figure similar to Fig. 5 for (eq10,21).
Such picture will not bring anything: we tried it. The improvement of a few % is not visible in the qualitative charts, such as fig. 5. This was the main reason why we did not recommend this approach unequivocally: the improvement was not landslide.

TECHNICAL CORRECTIONS:

Typo:
Equation4 : the term ρ_a is not defined
P27952-l24 : 'repeated' is repeated 2 times.
P27953-l4 and l13: I think the reference to Eq. 12 is instead Eq. 13.
P27953-l5: '....this fit for ...' ?
All corrected

Figures:
For the sake of clarity, a reference to the algorithms used [ie (eq10;13), (eq10;19), or (eq13;21)] could be added to each caption.
Done

COMMENT ON THE ANSWER TO THE NOTE POSTED BY EDWARD HYER.

'*the main message stays: during the calibrationof the formula, the value of FRP is directly related to the injection height Hp, thus taking into account the contribution of both sensible and latent heat. An implicit assumption behind this step is that the ratio of these contribution is about constant - or that the contribution of the latent heat is noticeably smaller than that of the sensible heat.*'

As reported by Rio et al 2010, the effect of the latent heat in the plume behaviour (e.g. pyroconvection) is due to the ambient water vapour entrained in the plume and not the water vapour injected by the fire (i.e. from the combustion).
We reformulated the discussion and made it more accurate. However, at present it is hard to comment on the specific details of the Freitas's model. As said above, we are going to initiate an intercomparison exercise, which will hopefully produce something unequivocal.

'*There are two indirect hints that the second may be closer to reality. Firstly, computations with BUOYANT (no accounting for humidity) appeared the second-best after our formula. The model failed only for few high plumes where the dry-plume assumption is indeed wrong. Secondly, the model of Freitas et al (2007), which shows comparable contributions of sensible and latent components, seems to over-estimate the heights, at least the fraction of the plumes reaching the free troposphere (we discussed it in the paper).*'
I think that the overestimation of the Freitas model is more likely related to the entrainment scheme which mainly controls the injection of water in the plume and so the contribution of latent heat. This scheme can be depend of fire characteristics and ambient condition. Therefore, a fix contribution of latent heat is certainly unrealistic.

We agree.